cussion which I contemplate for the years in question would probably occupy one computer for a year and a half, involving an outlay of about 70*l.*"

ON THE DARK PLANE WHICH IS FORMED OVER A HEATED WIRE IN DUSTY AIR[1]

IN the course of his examination of atmospheric dust as rendered evident by a convergent beam from the electric arc, Prof. Tyndall noticed the formation of streams of dust-free air rising from the summits of moderately heated solid bodies (*Proc. Roy. Inst.*, vol. vi. p. 3, 1870). "To study this effect a platinum wire was stretched across the beam, the two ends of the wire being connected with the two poles of a galvanic battery. To regulate the strength of the current a rheostat was placed in the circuit. Beginning with a feeble current, the temperature of the wire was gradually augmented; but before it reached the heat of ignition, a flat stream of air rose from it, which, when looked at edgeways, appeared darker and sharper than one of the blackest lines of Fraunhofer in the solar spectrum. Right and left of this dark vertical band the floating matter rose upwards, bounding definitely the non-luminous stream of air." . . .

"When the fire is white hot it sends up a band of intense darkness. This, I say, is due to the *destruction* of the floating matter. But even when its temperature does not exceed that of boiling water, the wire produces a dark ascending current. This, I say, is due to the *distribution* of the floating matter. Imagine the wire clasped by the mote-filled air. My idea is that it heats the air and lightens it, without in the same degree lightening the floating matter. The tendency, therefore, is to start a current of clean air through the mote-filled air. Figure the motion of the air all round the wire. Looking at its transverse section, we should see the air at the bottom of the wire bending round it right and left in two branch currents, ascending its sides, and turning to fill the partial vacuum created above the wire. Now as each new supply of air, filled with its motes, comes in contact with the hot wire, the clean air, as just stated, is first started through the inert motes. They are dragged after it, but there is a fringe of cleansed air in advance of the motes. The two purified fringes of the two branch currents unite above the wire, and, keeping the motes that once belonged to them right and left, they form by their union the dark band observed in the experiment. This process is incessant. Always, the moment the mote filled air touches the wire, the distribution is effected, a permanent dark band being thus produced. Could the air and the particles under the wire pass *through* its mass, we should have a vertical current of particles, but no dark band. For here, though the motes would be left behind at starting, they would hotly follow the ascending current, and thus abolish the darkness."

Prof. Frankland (*Proc. Roy. Soc.*, vol. xxv. p. 542), on the other hand, considers that what is proved by the above described observations is that "a very large proportion of the suspended particles in the London atmosphere consists of water and other volatile liquid or solid matter."

Last summer (1881) I repeated and extended Tyndall's beautiful experiment, not feeling satisfied with the explanation of the dark plane given by the discoverer. Too much stress, it appeared to me, is placed upon the relative lightening of the air by heat. The original density is probably not more than about 1/1000th part of that of the particles, and it is difficult to see how a slight further lightening could produce so much effect. In other respects, too, the explanation was not clear to me. At the same time I was not prepared to accept Prof. Frankland's view that the foreign matter is volatilised.

The atmosphere of smoke was confined within a box (of about the size of a cigar-box), three of the vertical sides of which were composed of plates of glass. A beam of sunlight reflected into the darkened room from a heliostat was rendered convergent by a large lens of somewhat long focus, and made to pass in its concentrated condition through the box. The third glass side allowed the observer to see what was going on inside. It could be removed when desired so as to facilitate the introduction of smoke. The advantages of the box are twofold. With its aid much thicker smoke may be used than would be convenient in an open room, and it is more easy to avoid draughts which interfere greatly with the regularity of the phenomena to be observed. Smouldering brown paper was generally used to produce the smoke, but other substances, such as sulphur and phosphorus, have been tried. The experiment was not commenced until the smoke was completely formed and had come nearly to rest. In some respects the most striking results were obtained from a copper blade about ¼-inch broad, formed by hammering flat one end of a stout copper rod. The plane of the blade was horizontal, and its length was in the line of sight. The unhammered end of the rod projected from the box, and could be warmed with a spirit-lamp. The dark plane was well developed. At a moderate distance above the blade it is narrow, sometimes so narrow as almost to render necessary a magnifying glass; but below, where it attaches itself to the blade, it widens out to the full width, as shown in the figure

Whether the heated body be a thin blade or a cylindrical rod, the fluid passes round the obstacle according to the electrical law of flow, the stream-lines in the rear of the obstacle being of the same form as in front of it. This peculiarity of behaviour is due to the origin of the motion being at the obstacle itself, especially at its hinder surface. If a stream be formed by other means, and impinge upon the same obstacle without a difference of temperature, the motion is of a different character altogether, and eddies are formed in the shadow.

The difference of temperature necessary to initiate these motions with this dark plane accompaniment is insignificant. On July 20, 1881, a glass rod, about ¼-inch in diameter, was employed. It was heated in a spirit-lamp, and then inserted in the smoke-box. The dark plane gradually became thinner as the rod cooled, but could be followed with a magnifier for a long time. While it was still quite distinct the experiment was stopped, and on opening the box the glass rod was found to be scarcely warmer than the fingers. It was almost impossible to believe that the smoky matter had been evaporated.

In order to test the matter more closely, smoke was slowly forced through a glass tube heated near the end pretty strongly by a spirit-lamp, and then allowed to emerge into the concentrated sunshine. No distinct attenuation of the smoke could be detected even under this treatment.

It is not necessary to dwell further upon these considerations, as the question may be regarded as settled by a decisive experiment tried a few days later. The glass rod before used was cooled in a mixture of salt and ice, and after wiping was placed in the box. In a short time a dark plane extending *downwards* from the rod, clearly developed itself and persisted for a long while. This result not merely shows that the dark plane is not due to evaporation, but also excludes any explanation depending upon an augmentation in the difference of densities of fluid and foreign matter.

The experiment was varied by using a U-tube through which cooled water could be made to flow. When the water was not very cold the appearances were much the same as with the solid rod; but when by means of salt and ice the tube was cooled still further, a curious complication presented itself. Along the borders of the dark plane the smoke appeared considerably brighter than elsewhere. Sometimes when the flow was not very regular it looked at first as if the dark plane had been replaced by a bright one, but on closer examination the dark plane could be detected inside. There seems no doubt but that the effect is caused by condensation of moisture upon the smoke due to the chilling which the damp air undergoes in passing close to the cold obstacle. Where the fog forms more light is scattered, hence the increased brightness. That the fog should not form within the smoke-free plane itself is what we might expect from the interesting observations of Aitken.

With respect to the cause of the formation of the dark plane, the most natural view would seem to be that the relatively dense particles are thrown outwards by centrifugal force as the mixture flows in curved lines round the obstacle. Even when the fluid is at rest a gradual subsidence must take place under the action of gravity; but this effect could at first only manifest itself at the top where the upper boundary of the gas prevents the

[1] Paper read at the Royal Society, December 21, 1882, by Lord Rayleigh, F.R.S., Professor of Experimental Physics in the University of Cambridge.

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entrance of more dust from above. It is known that air in a closed space will gradually free itself from dust, but the observation of a thin dust-free stratum at the top of the vessel is difficult. If we conceive a vessel full of dusty air to be set into rapid rotation, the dust might be expected to pass outwards in all directions from the axis, along which a dust-free line would form itself. I have tried this experiment, but looking along the axis through the glass top of the vessel, I could see no sign of a dark line, so long as the rotation was uniform. When, however, the vessel was stopped, a column of comparatively smoke-free air developed itself along the axis. This I attributed to the formation of an inward flow along the top of the vessel, combined with a downward flow along the axis after the manner described and explained by Prof. James Thomson, so that the purified air had been in intimate proximity with the solid cover. It would almost seem as if this kind of contact was sufficient to purify the air without the aid of centrifugal force.

The experiments made hitherto in order to elucidate this question have given no decisive result. If the thin convex blade already spoken of be held in the smoke-box in a vertical instead of in a horizontal plane, the lines of motion are much less curved, and we might expect to eliminate the influence of centrifugal force. I have not succeeded in this way in getting rid of the dark plane; but since under the magnifier the curvature of the motion was still quite apparent, no absolute conclusion can be drawn.

ON THE MORPHOLOGY OF THE PITCHER OF "CEPHALOTUS FOLLICULARIS"[1]

THE brief, but most interesting, memoir on this subject read by Prof. Alexander Dickson before the Botanical Society of Edinburgh on March 10, 1881, was the first to throw any clear light upon the obscurity which had previously enveloped it. The conclusions at which he arrived seemed to be fully sustained by the facts which he then published; but since there are still botanists who do not fully accept those conclusions, any independent evidence bearing upon the problem of the morphology of these curious pitchers may be worth recording.

The publication of Prof. Dickson's memoir caused me to watch the growth of my plants of Cephalotus with increased interest. From time to time abnormal leaves have made their appearance, which seemed to afford more or less support to the views which the Professor entertained. This spring one of my plants has developed a leaf the growth of which I have watched. When this leaf first became visible it bore no indication of being other than an ordinary leaf of the plant, but its upper surface soon exhibited a somewhat shrivelled appearance, like that of a leaf distorted by the action of Aphides. It soon became evident that this disturbance was but the commencement of the process of pouching described by Prof. Dickson. That which at first appeared to be a mere distortion of the surface of the leaf soon deepened into a considerable depression, which became more considerable day by day until the leaf reached the condition represented in my figures 1, 2, and 3. Fig. 1 represents the upper, 2 the lateral, and 3 the inferior surface of this leaf.

From the beginning of its growth *a* was the unmistakable, somewhat cuspidate apex of the leaf, as it was also the distal end of the prominent ciliated ridge, *b*, the obvious precursor of the middle dorsal wing, which forms so conspicuous a feature of the normal pitcher. It will be seen in Fig. 2 that this ridge only extends downwards to the point *c*, whilst *d* was evidently the fundus of the enlarging pouch, relations which approximate closely to what characterise these portions of the perfect pitcher. On the under surface of the leaf (Fig. 3) we find this middle wing extending downwards from *a*, flanked on either side by a smaller, slightly curved ridge, also ciliated, the two unquestionably representing the lateral wings of the normal organism. It is perfectly clear that the peripheral outlines of the figures 1 and 3 represent the true primary margins of the leaf from the point *a* to the base of the petiole *e*. The upper half of this margin is abundantly ciliated, the hairs becoming more scanty as we approach the lower half of the leaf.

Figs. 1 and 2 show the form of what obviously represents the lid, *f*, of the true pitcher. In its essential features it accords with those figured by Prof. Dickson, who correctly recognised its true homology. As in his Fig. 5, this lid is two-lobed, its central indentation, *g*, separating two triangular lobes. This arrangement corresponds substantially with what exists in the normal pitcher, only in the latter the lobes are large and rounded instead of being small and triangular. The free margin of this rudimentary lid is abundantly ciliated, as in the perfect pitchers.

Thus far my specimen only confirms and illustrates the conclusions arrived at by Prof. Dickson, viz. that the pitcher is merely a depression in the upper surface of the leaf, of which the petiole *e* is identical morphologically with the terete petiole of the true pitcher, whilst the lid, *f*, is an outgrowth of the

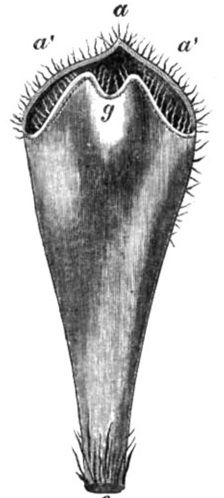

Fig. 1.

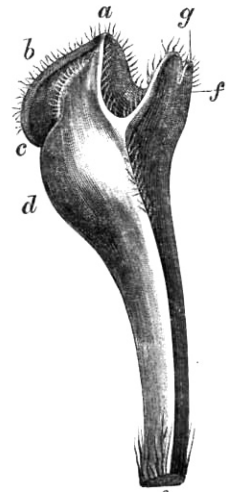

Fig. 2.

upper surface of the leaf from the proximal margin of that depression.

Prof. Dickson was not able to decide with absolute certainty which part of the matured normal pitcher represented the primary apex of the leaf. In his abnormal specimens, as in mine, that apex coincided with the apex *a* of the middle dorsal wing. As is well known, in the perfect pitcher the entrance into the pitcher is bounded by a thick, involuted, toothed rim, to which the apical point of the dorsal wing is external. The Professor

Fig. 3.

was uncertain whether the apex *a* of the wing coincided with the true apex of the leaf, or whether that apex is hidden in the involuted margin of the pitcher. He inclines, however, towards the former view, and I have the conviction that he is right. The two *ciliated* margins, *a' a'*, of Fig. 1, are obviously the two lateral margins of the anterior portion of the normal leaf, demonstrating clearly that the point *a* is its apex. In the true pitcher these margins have lost their ciliæ, a few prominent teeth being substituted for them, and become thickened at their inner side

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